STILL

September 12, 1955

Dear Jack:

We were delighted to hear from you, and tosee that our paths may be crossing again even in a remote scientific sense. I am particularly happy to learn of your technical progress in the isolation and characterization of yeast mitochondria.

I am not certain that there is any fundamental conflict between you and Ephrussi, though his technique may not have been so successful in demonstrating the full enzymatic equipment of the particles. Several reviewers have misquoted him as maintaining that the petites lack the mitochondrial granules, and that these are certainly equivalent to the cytoplasmic genetic particle. He has been careful to point out that these may be distinct entities, though Slonimski has gone somewhat further in adducing evidence for their identity. For myself, I have never been happy with that theory: after all, the "segregational" petite mutant confers an identical phenotype of respiratory defect, although the genetic particles are still intact. I had thought the only way to identify the "genetic particle" was to try to isolate it, and introduce it artificially, into a petite cell— which is, of course, what Syd has been working at. I hope you and he will be able to get together on this problem. Whether or not the mitochondria are the genetic particles, one would not have much hope for a fractionation technique that did not at least preserve the former.

Since Syd was here last year, we have had an Australian lad (Robert F. Wright) whom you may know, doing some genetic work on yeast, related to the same problem. He has been trying to confirm the cytoplasmic hereditary determination of the petite character by studies on "dikaryons" which occur, in some strains, as incidents of conjugation. All sorts of side issues have come up (e.g., the incidence of petites in irradiated cultures) to conspire to frustrate a simple decision, and so far only a few segregants from petite x normal have been tested. These few, remarkably, have been still petite, and therefore do not accord with the theory of cytoplasmic determination. However, a single normal segregant (of an originally petite genotype, would turn the picture about, so the conclusion is not decisive. We have been surprised (assuming the correctness of the cytoplasmic theory) that there should be incomplete mixing of the cytoplasm in conjugating pairs, but this would account for the results so far. At any rate, Wright and Hans Ris have been looking at the yeasts cytologically, and as you would have predicted, find little if any morphological difference between petite and normal, except for a difference in reduction of Janus Green, in the particles of the petite strain.

If we do visit Australia, we shall of course look forward to seeing you.

Ypurs sincerely,

Joshua Lederberg